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EDITORIAL

A WORD ON SYNERGY.

FEW anæsthetists nowadays limit themselves to one agent when they administer an anæsthetic. Even if they do not use more than a single one at the operation itself they have generally beforehand employed one if not several adjuvants. It is a good thing in practice to understand completely the principles on which one's practice is founded, and it appears that there is often more than a little confusion as to the exact meaning of the doctrine of synergism. Synergy, or synergism, means really simply the co-operative action of two or more agents. In anæsthetic parlance it has come to mean more than this, viz., "the one-sided or reciprocal augmentation of one drug by another" (Meyer and Gottlieb). That such a form of action really exists there is no doubt, but in practice its existence is often assumed on the most slender evidence or on none at all. The reality of synergistic action was shown by Meltzer when he demonstrated that after the administration of a very small amount of ether, insufficient to cause anæsthesia, an inefficient amount of magnesium is injected intramuscularly, a profound anæsthesia follows, which can be maintained for several hours. This is true synergism, and the same kind of effect may be brought about by the co-operative action of two agents each of which is itself a narcotic. The

sum of the combined action of two or more narcotics administered simultaneously, or shortly after each other, produces a much more powerful effect than when a total equivalent quantity of either of the narcotics had been administered alone. This increased action is particularly marked when the two narcotics are different cell receptors or belong to different chemical series, in which case the two drugs seem to potentiate each other, and a dose of any one drug acts much more markedly when given in frequent small doses than when administered in a single dose. These are, we believe, the propositions laid down in what is known as Burg's law. Its practical application, in the use of magnesium sulphate hypodermically and intramuscularly for the potentiation of ether and nitrous oxide, has been made familiar by Gwathmey's experimental and clinical work. A further feature of synergy is claimed, viz., that the toxic effect of the drugs which are combined increases much more slowly than the therapeutic effect does. This has been especially demonstrated for antiseptics by Bouchard and Lépine and is mentioned in *Anesthésie et Analgésie* (April 1938) in the course of a discussion of what is styled the English "cocktail" method of anæsthesia. Here, too, is pointed out what is almost certainly true and is often not borne in mind, that while some anæsthetics reinforce each other others are simply added on each to each. It is in fact an extremely difficult problem to solve when several anæsthetics are employed on the same subject, whether anæsthetic synergy or mere accumulation of agents is actually brought about.

CONCERNING SPINAL ANALGESIA

(I)

By W. ETHERINGTON-WILSON, F.R.C.S.

IN the *British Journal of Anæsthesia*, April 1938, pp. 117 to 121, appears an article by Van der Post which needs comment. The opening headlines catch the eye at once: "Three disturbing cases of spinal anæsthesia administered according to the Etherington-Wilson technique . . . which have rather badly disturbed my confidence in the Etherington-Wilson technique." The writer then proceeds really to justify the safety of the technique, by (1) using small doses of 1/1500 percaine (10 c.c. in all three cases) for a safe number of seconds; (2) saying that "in all three cases anæsthesia was perfect at least as far as the umbilicus"; (3) quoting others, i.e., "there is probably a considerable margin of safety after 80 seconds of sitting the patient up" (Jarman). That ten cases were allowed to sit up for one to two minutes (Preissecker of Vienna) "one would conclude that it is quite safe to sit them up for any length of time," etc.; (4) saying that if it is true as Preissecker thinks, that the anæsthetic fluid is so rapidly absorbed by the roots that when it reaches the fifth dorsal level there is no more drug left to be absorbed . . . then it would appear that there was some definite contra-indication to a spinal anæsthetic."

To summarise: Van der Post's unfortunate opening remarks immediately condemn the technique to a casual reader. Perhaps this was not intended but such a construction can and certainly will be taken by some. Thereafter he gives plenty of reasons for the safety of the technique, but really wonders whether "the safety factor of spinal analgesia or of percaine . . . is smaller than is generally believed." Does he really think that if he had used any other technique, used percaine, arrived at the umbilical level and carried out the same procedures after induction, that none of these cases would have disturbed him? The results would have been the same, one fright and two deaths. Are

not the opening passages of the article unfair and misleading? I suggest that Van der Post is really questioning the safety of spinal analgesia, not the technique. I feel sure that his induction of the spinal alone need not in any way disturb his confidence.

I feel very diffident about defending my own work, but think it necessary after reading this account, and also because a deep-rooted prejudice exists in the medical profession about the use of hypobaric solutions and the vertical position. Much of this is due no doubt to the instructions in large letters on the boxes containing spinocaine ampoules. During lectures in various places, including Canada and the States, the same statements have nearly always been forthcoming, namely, "I don't know how you dare induce in the sitting position when using a light solution; does not the solution fly up to the medulla? Are you not afraid of fainting in this position from a fall of blood-pressure?" These questions are easily answered and, in addition, I always recommend mock analgesia in spinal glass canals to doubters. Nobody can become a good, competent and confident spinal anæsthetist rapidly, unless they can visualise the behaviour of solutions after injection; this statement bears repetition because very few, I know, have bothered to obtain a glass tube. It is useless to argue that a glass tube does not in the least resemble the human spinal canal with its contents. I can assure Dr. Van der Post that the vertical position can claim the following advantages.

(1) Slow ascent. The higher the hypobaric solution gets, the slower it travels. The rate above the fifth thoracic segment must be about 15 seconds to an inch (10 c.c. percaine sp. gr. 1.003). The latter amount travels just twice as quickly at an angle of 45°.

(2) Watery solutions have an even ascent and the mixing is thorough and automatic.

(3) It is a method of control and the timing of any solution can be worked out. By control I do not imply that the pubes or umbilicus can be hit off every time exactly, because many other factors may play a part including, as Van der Post mentions, the variation in the sp. gr. of the canal spinal fluid.

(4) It definitely allows of minimal dosage. It is not a volumetric method. May I quote a sample of the latter: "We have been impressed by the 'X' method and have had no failures using 20 c.c. of 1/1500 percaine."

The more you put in the better the spinal, is not a slogan of mine. After induction the patient is sloped steeply for three to four minutes. The inclination used for operation, in cases not requiring Trendelenburg, is about 8 degrees, an angle at which the patient just does not slide up the table. A similarly sloped trolley takes the patient back to the ward. In bed a very steep slope is used, to allow for bed sagging, for about six hours. This is subsequently changed to a lesser and more comfortable incline for another 18 hours. These remarks refer, of course, to cases where there is no contra-indication for the head down position.

In further defence and confidence in the technique, I have been using spinocaine in the sitting position, with certain precautions, very successfully. I consider it safer than the Pitkin method of inclinations. A small series of babies have also been given spinals (percaine 1/2500) without any suggestion of spinal mishap.

Dr. Van der Post in his frank paper, which will serve a useful purpose, asks for criticism and assistance. I hope others will try and help, and assist, in fact, all of us. A few points need some mention.

The types of cases mentioned were: First, Cæsarean; second, Cæsarean, died; third, ectopic gestation, died. No mention is made in Case 3 as to whether there was any loss of blood before or during operation. If Cæsarean sections were all done as a routine under spinal analgesia and without particular regard to danger, there is, I believe, trouble waiting round the corner. The same may apply, to a lesser extent, to the removal of huge abdominal tumours. Anxiety is still more likely to be caused if a pregnant woman is already ill (Case 1, blood-pressure 118/70, looked ill, etc.), or already exhausted, (Case 2, native in labour 15 hours). The special feature of such cases, which does not occur in other operations, is the rapid loss of a large quantity of blood and the equally sudden release of abdominal tension

by the removal of a large tumour. Consider the probable sequence of events in such a case:

(1) *Ab initio*, an ill or exhausted patient: depression of centres.

(2) Premedication, especially morphia, etc.: drug action added.

(3) Spinal analgesia, probable fall in blood-pressure: nutrition of centres impaired.

(4) Absorption of percaine: further depression of centres.

(5) Inadequate analgesia (if only to tenth segment): traumatic shock added.

(6) Sudden loss of much blood: further depression of centres.

(7) Loss of abdominal tension: further depression of centres.

(8) Administration of CO_2, possibly: temporary flogging and stimulation of respiratory centre, followed by reactionary exhaustion.

(9) Moving patient during the lowest ebb period, before recovery commences: may result in cessation of respiration.

Let us consider shortly some of these events and possibilities. We are, it seems, concerned chiefly with the condition of the medullary centres in these cases. The presence of an unassessed myocardium is another bugbear in anæsthetics of any kind. Spinal analgesia is not a panacea for ill patients, though it may at times be much safer than a general. It may be dangerous in those cases where the blood-pressure has become lowered recently through illness. It is not unreasonable to suppose it unsafe in a woman whose centres and heart are tired after some hours of unsuccessful effort, straining and pain.

I have come to regard premedication from two opposite points of view. Firstly, that it is essential, speaking generally, in spinal block; secondly, that it is dangerous. I seldom use morphine preparations and always advise against them (all the three cases were given morphia). It is surely an exceedingly important point to realise that over-drugging

may be dangerous, particularly perhaps to the respiratory centre, and when it is proposed to produce a condition such as we see in spinal analgesia. I have repeatedly pointed out that it is not necessary for patients to be asleep and difficult to rouse for the production of loss of fear and amnesia: these are overdrugged. Few take into consideration the additional effect of the circulating percaine. Many an apparently wide-awake patient has gone soundly to sleep 15 minutes after induction. I would suggest, then, that premedication, in serious emergency conditions should be minimal; depressants and powerful drugs like morphia, avertin, nembutal, evipan, etc., avoided altogether. I now prefer paraldehyde P.R. to scopolamine, but use both.

Possible fall of blood-pressure from nerve block and the additional drugging effect of absorbed percaine speak for themselves with regard to depression and diminished nutrition of the medullary centres. CO_2 must require some consideration in these depressed types of case. My impression is that its stimulation may be temporary and possibly dangerous in low, drugged states of the respiratory centres, when used as an emergency measure. I would be interested to hear what others say on this point.

The higher spinal inductions are better not used for risky cases in which operation is completed rapidly and the patient moved off the table within half an hour; unless the theatre is prepared to keep them for a further period of time—say another 15 to 30 minutes, depending on the fullness of the pulse. The reason for this is difficult to understand, but I have seen alterations in the colour and the pulse of the patients, on movement. In some ill cases I have definitely ordered the patient to be left alone for a period and kept warm on the sloped theatre table. It would seem possible that a vital centre, depressed, fatigued and drugged is not amenable to any rough movement or even temporary alteration in posture of the patient. This lowered condition is seen at its maximum usually about 20 to 30 minutes after injection. The respiratory centre is not, I think, supplied with a continuous surge of energy, which will overcome all ordinary obstacles and repeated battering threatening its stability.

Van der Post states that all three cases were analgesic at least to the umbilicus. If the level was no higher than this then the question of shock may also have to be added from handling unblocked nerve tissue.

The Experimental Side.

Van der Post mentions the rate of ascent of percaine. Actually there are many factors which influence the rate of ascent. (1) Specific gravity, (2) amount injected, (3) alcohol containing solutions, (4) rate and method of injection, (5) calibre of needle used, (6) width of spinal canal and volume of canal spinal fluid. These are some of the reasons which make it difficult to demonstrate a prearranged level of analgesia in every case, especially in the low segments. But it is of some use to know that we have enough *control* to get within a segment or so of our aim. High spinals are very easy to get by my technique and much more accurate, because a few seconds more makes very little difference in the higher reaches of the canal, owing to the very much slowed up rate of travel of the ascending percaine. In practice, therefore, I see no danger from any of these influences (we are not concerned with the alcoholic solutions). I have had very successful spinals in jaundiced patients and in anæmia, but cannot say what the specific gravities of the canal spinal fluid were in these cases. I do allow for the width and length of the canal, in dosage and timing, and this, of course, applies mostly to lightly built females. From the evidence of experiments, with balloons attached to the upper end of a glass canal, I came to the conclusion that pressure effects do not influence to any practical extent the ascent of a light fluid. It can only have its small effect on account of the increased volume of canal spinal fluid if the pressure is high, which would tend to slow upward progress on account of there being more canal spinal fluid for admixture. The same argument applies to a very wide canal.

Diffusion is mentioned by Van der Post. I take it that he wants to know if percaine, after the umbilical (tenth dorsal) segment has been reached and established, can slowly and insidiously "diffuse" into dangerous high zones.

This is impossible because (*a*) after about eight to ten minutes this level of established analgesia cannot be altered, (*b*) we know that absorption from the canal is fairly rapid, (*c*) a long continued Trendelenberg is adopted a few seconds after injection, (*d*) could such a diluted solution of percaine cause serious inhibition of centres. (I have injected percaine into the cisterna magna of a hydrocephalic baby without a suggestion of anxiety).

Those of us who use spinals to any extent must be impressed by the necessity of team work and apparatus. The outstanding points are worthy of mention, for they have a definite bearing on safety measures. The anæsthetist, it is assumed, knows all about the subject and conducts the spinal analgesia. The surgeon must have a good knowledge of the subject otherwise he may be rough, hurried, very talkative to patient or staff during operation, and may even demand that the table be raised. The doctor of the case, in his kindly concern for his patient, can be a perfect nuisance, unless he is made to understand (he cannot guess) that questioning, reassuring or patting the patient on the cheek is prohibited.

The sister has great responsibilities. She must learn to acquire the knack of knowing when a patient is sufficiently drowsy. She often gets worried because they will not sleep or can be too easily roused and appear very alert, and gives a second dose which was left at her discretion. She must understand the reasons for after-care which begins when operation is completed and carry them out to the letter.

Summary.

1. An attempt has been made in defence of the safeness of the technique.

2. Some points have been discussed with regard to the cases mentioned by Dr. Van der Post.

3. Mention is made of some physical factors raised.

CONCERNING SPINAL ANALGESIA

(II)

A CRITICISM.

By E. FALKNER HILL, M.D.

MR. C. W. H. VAN DER POST has asked for a candid criticism of the technique which he used in three cases of spinal anæsthesia, two of which were fatal and the other a very "near thing". Here is one.

He summarises the "advantages" of the technique which he used as follows:—

1. Ease of performing lumbar puncture in the sitting position.

2. Comfort to the patient in avoiding a forceps-like grip over the neck and round the knees and in avoiding, too, the turning of the patient on his face and then on to his back.

3. Easy control of the level of anæsthesia by the diffusion rate of the anæsthetic fluid.

4. The quickness with which the patient can be got ready for operation.

5. The use of a drug which in the concentration used is decidedly less toxic than the numerous novocain derivatives and one, too, that does not contain any alcohol.

Every one of these supposed "advantages" with one exception is, in fact, a disadvantage. Let us consider them in the order given.

1 and 2. Though at first sight it would seem to be easier to enter the theca with the patient in the sitting position, I think the majority of men who have had extensive experience prefer the lying down position. There are two good reasons for this, firstly, a patient who has had morphia, and this, of course, applies with greater force to the more heavily drugged, is more comfortable, safer, less likely to faint lying down than sitting up. Secondly, a "forceps-like grip" is not only unnecessary but undesirable in that it tends to

stretch and therefore render tense the interspinous ligament, and so it is slightly more difficult to find the exact spot to make your puncture and slightly more difficult to make it. The tenseness of the interspinous ligament is somewhat accentuated in the sitting up position. Pitkin, though the target for much exuberant wit, taught and taught with no uncertain sound "Never give a light solution in the sitting up position, sooner or later you'll regret it." I, for one, would like to underline that advice. I've yet to see a death on the table following the injection of a heavy solution. On the other hand I've seen three deaths following the use of light solutions, though I've given seven times as many heavy as light solutions.

Owing to the fact that a patient under a spinal anæsthetic has been to a large extent deprived of his mechanisms of adjustment, it is better to move such a patient as little as possible. The side position using a heavy solution is therefore the best of all the techniques in this respect, as the patient is simply rotated through an angle of 90° to get him from the side position to the back position.

3. These three cases demonstrate in the most dramatic way possible that the control of light solutions in the sitting up position is "not easy". In case 3, 10 c.c. of light percaine with the patient sitting up for 15 seconds almost paralysed respiration in 15 minutes and quite paralysed it in 20 minutes. In case 2, the same dose with the patient sitting up somewhat longer, namely, 20 seconds, did not paralyse respiration in the first 40 minutes, possibly did not even weaken it. In case 1, the same dose sitting up for the same time certainly did not paralyse respiration but it seems probable that there was some weakening of the respiration. Here, then, we have three different results following the same technique, alike only in that they were all, either dangerous or fatal. A more powerful condemnation of the method it would be difficult to find.

4. It is usual to begin the operation five minutes after the anæsthetic is given if that anæsthetic is one of the novocain series. I have known more than one operation started within two minutes. I should be surprised if the technique here referred to can improve on that.

5. In the doses commonly used up to 0.3 grammes novocain and 0.03 grammes decicain no toxicity has been noticed. In experiments on cats it is necessary to inject 18 times the full spinal dose into the jugular vein to paralyse respiration and even this enormous dose is recovered from by two minutes' artificial respiration. To put it more precisely, 10 mgm. of novocain injected into the cisternamagna of a cat will kill it in one minute and a half by paralysis of respiration. If rescued by timely artificial respiration the same cat will receive 7½ mgm. of novocain into the jugular vein every minute for 24 minutes before respiration is paralysed. Three hundred mgm. of novocain have been injected into the median cephalic vein of a human being in the course of 10 minutes without any subjective symptoms making their appearance and with no fall of blood-pressure. Sebrechts makes a similar statement about percain, viz., "The injection into a vein of 20 c.c. of percain 1.1500 without adrenalin causes no disquieting symptoms whatever." So the question of toxicity does not arise.

When we come to consider the individual cases it is obvious that in case 3 the anæsthetic went "too high". "The injection was completed at 2.20, but at 2.35 the patient's *alæ nasi* were functioning to excess and respiration was irregular and sighing in character". There are not sufficient details about case 2, but as the patient died some 45 minutes after the injection had been completed, the death cannot be attributed solely to its action on the respiration. Case 1 showed definite signs of respiratory failure but only after 30 minutes. It is probable that the anæsthetic was in part responsible for a weakening of the respiration, but if so why the delay in making this action manifest? It will be noticed that the patient received 1/3 gr. of omnopon and 1/150 grain of scopolamine. Though many a healthy patient will stand such a dose and survive, it is too depressing for a "bad risk".

The patient's life depends on sufficient cardiac action, which in turn derives from the minute volume of the circulation and the oxygen tension in the coronary arteries of the heart muscle wall. The only way in which a spinal anæsthetic can interfere with the minute volume of the

circulation of the blood in the coronary arteries and the oxygen tension therein is by weakening or paralysing respiration. In the case here mentioned it was merely weakening respiration. If the respiratory centre is already depressed by sedatives the more it is so depressed the less the spinal anæsthetic will have to do to put it out altogether. But even in cases where the respiratory centre or phrenics are not touched by the anæsthetic, a depressed centre means some anoxæmia owing partly to the Trendelenburg position partly to the unequal and inefficient expansion of different parts of the lungs. Paralysis of the lower vaso-constrictors leads to a fall of blood-pressure unless compensated for by the over-activity of the upper and unparalysed vaso-constrictors, and this over-activity is the less likely in a "bad risk" than in the comparatively healthy individual. With a light solution the upper vaso-constrictors are more likely to be paralysed than with a heavy solution, leading to a further fall in blood-pressure, it is almost certain they were in this case. The fall of blood-pressure is apt to cause feeble heart action which in turn means a poor supply to the medullary centres with still feebler respiration and a greater degree of anoxæmia. This vicious circle leads gradually to such diminution of the minute volume of the circulation and the oxygen tension in the coronary arteries that sufficient oxygen does not reach the cardiac muscle cells to sustain their activity. It is very evident that a depressed respiratory centre has played a large part in this alarming experience.

Certain definite rules follow from a consideration of the facts.

1. Use heavy solutions rather than light. Because if by chance you have given too big a dose it will expend itself harmlessly on some upper posterior dorsal roots. Whereas if you have made a similar mistake with a light solution it will attack the upper dorsal anterior roots and possibly the phrenics. In the first case you will get an unnecessary, undesirable fall of blood-pressure, and in the second a paralysis of the phrenics.

2. Never give light solutions in the sitting up position.

3. As we have found in cats that a dilute solution of large bulk is more likely to paralyse respiration than an even

larger dose of the drug in a small bulk comparatively concentrated solutions are safer than large bulk dilute solutions.

4. Don't give powerful respiratory sedatives beforehand. You may need an active respiratory centre to prevent a fatal anoxæmia.

5. Be prepared to give oxygen intratracheally at a moment's notice, and do it before it is too late.

6. Unless the indications for a spinal anæsthetic are very definite don't use it in fat, flabby, or dehydrated patients. The objection sometimes raised to the use of heavy solutions, that they may go too high if the Trendelenburg position is used, is not serious. For an upper abdominal section the table should be put in a slight Trendelenburg position (3°) with the head raised, while the injection is being made, and should remain in that position for a quarter of an hour, after that it is quite safe to increase it to any desired extent.

CONCERNING SPINAL ANALGESIA

(III)

TWO PERTINENT CASES AND SOME CRITICISM.

By H. BRENNAN, M.B.

WITH reference to Dr. Van der Post's article, the following cases are of interest:—

1. J.H., male, aged 71 years, was admitted to hospital on October 18th, 1936, suffering from acute retention of urine. He was of rather stout build and a chronic bronchitic, rhonchi and rales being audible all over the chest.

On October 22nd, 1936, I gave him 1.2 c.c. stovaine (Barker) in the sitting position. Anæsthesia was satisfactory and uneventful. The urologist performed a cystoscopy and decided to perform a perineal prostatectomy the following week. On October 29th, 1936, he was placed on the table at 2.10 p.m., having received atropine sulph. gr. 1/60 at 1.30, and ephedrine gr. 1½ at 1.55. His blood urea was 20 mgs. per 100 c.c. of blood and urea clearance 98.91 per cent.

I intended to give light percaine, using the Howard Jones' technique, and he was, therefore, placed in the right lateral position. I had considerable difficulty in introducing the needle, and after a few minutes I told the nurse to sit the patient up. With the patient in the sitting position I introduced the needle immediately in the third lumbar interspace, and then injected 12 c.c. of light percaine at room temperature, taking 15 seconds over the injection.

The patient was immediately put into 17 degrees Trendelenberg, the time being 2.25. At 2.28 he looked unhappy. At 2.29 he was cyanosed. The syringe was loaded with 10 c.c. of coramine and O_2 and CO_2 were given. By 2.31 no respiratory movements could be detected, though the accessory muscles were acting strongly. The coramine was given into the heart and rhythmic insufflation with O_2 and CO_2 carried on, but the patient was certainly dead at 2.40.

2. C.K., male, aged 52 years, was admitted to hospital on December 17th, 1937, complaining of hæmaturia. He was of rather stout build and had some degree of chronic bronchitis and emphysema. He gave a nine years' history of winter cough and dyspnœa. He had some very bad teeth but refused to have them extracted.

On December 22nd, 1937, he was given a spinal (stovaine) by a house surgeon. Anæsthesia was uneventful and cystoscopy and fulguration of a tumour were performed.

On January 26th, 1938, this anæsthesia and operation were repeated, both again being uneventful.

The urologist decided to make a suprapubic exposure and implant gold seeds, and for this purpose the patient was brought to the theatre and placed on the table at 2.0 p.m. on February 3rd, 1938. His blood urea was 31 mgs. per 100 c.c. blood, blood-pressure 180/105. He had received omnopon gr. 1/3, scopolamine gr. 1/300 at 1 p.m., and ephedrine gr. 1½ at 1.45 p.m.

He was placed in the right lateral position as I intended to give him light percaine, using the Howard Jones' technique. I had difficulty in inserting the needle, largely because the patient would not keep still, and I told the nurse to sit him up. I then introduced the needle without difficulty in the third lumbar interspace and injected 12 c.c. of light percaine at room temperature, taking 15 seconds over the injection. The patient was immediately placed in 20° Trendelenberg, the time being 2.15. At 2.20 the patient whispered "can't talk". His colour was fairly good, though no movement of the diaphragm was taking place, but there was a little intercostal action and marked action of the accessory muscles. I immediately started rhythmic insufflation with O_2 and CO_2, using a face piece. By 2.25 the intercostal movement had disappeared as well as the diaphragmatic. He was unable to whisper, but appeared conscious. The *alæ nasi* and supraciliary muscles were working frantically. Insufflation was continued, an even more agonised look appearing in his eyes if the rhythm was discontinued for a second or two. I reassured him, with far more confidence than I felt, that he would be perfectly all right, and told him not to try so hard to breathe.

At 2.38 he was becoming cyanosed, so to make the insufflation more effective, I passed a No. 8 Magill tube under direct vision. The cords were hanging wide apart and immobile. No air at all was passing. I packed some gauze into his mouth after connecting the tube with the gas machine (Webber), and continued the rhythmic hand squeezing of the bag. His colour became pink again, and he appeared happier.

At 2.50 his pulse deteriorated slightly and I gave him 3 c.c. of coramine intravenously. At 3.15 his pulse and colour were good and he looked comfortable so long as the insufflation was continued. At 3.20 he was able to move his legs slightly, though no respiratory movement could be detected. At 3.30 there was definite though still very slight intercostal and diaphragmatic movement, on momentarily discontinuing insufflation. At 3.45 I removed the gauze from his mouth, and in answer to a question he was able to phonate well enough for us to interpret his reply as "better". At 3.50 insufflation was discontinued though the tube was left in. At 4.0 diaphragmatic and intercostal movement, pulse and colour were all normal. Though the tube was still in the trachea, he was able to make us understand that he was "quite all right now". The tube was then removed.

He confirmed that he had been conscious all the time and very frightened. He said he had seen me push "that" —pointing to the laryngoscope—down his throat, but had not felt it. He had no subsequent ill effects from this experience, but insisted on going home without further treatment.

His urinary symptoms became worse and he was readmitted some weeks later. On May 19th, 1938, he received the same premedication as before—omnopon 1/3, scopolamine 1/300 and ephedrine gr. 1½. I promised him there would be no repetition of his previous experience and gave him 14 c.c. of light percaine, using the Howard Jones' technique. Anæsthesia rose to D6. Throughout the operation—a difficult laparotomy and double ureterostomy—his condition remained perfectly normal and satisfactory.

To me, these cases are an unanswerable argument against

the administration of a spinal anæsthetic of a lower specific gravity than that of the canal spinal fluid to a patient in the sitting position. I have always considered such a technique to be intrinsically unsound, and on the two cases quoted I used it against my better judgment.

Dr. Van der Post says that the Etherington-Wilson technique has five advantages.[1] On analysis I do not find them very real. 1. "Ease of performing lumbar puncture in sitting position." It is only very occasionally one has difficulty in performing this manœuvre in the lateral position. If in an odd case the canal can only be tapped in the sitting position, there is still the choice of:

(i) Replacing the patient in the lateral position with the needle in situ.

(ii) Giving a heavy solution. (I have just given a series of 50 patients heavy percaine for upper abdominal operations, in each case giving the percaine with the patient in the sitting position. In only one case was anæsthesia too low).

2. "Comfort to patient in avoiding forceps-like grip over neck and round the knees and in avoiding, too, the turning of the patient on the face and back again."

It is certainly essential not to turn into the prone position the obese, the pregnant and the obstructed. If one wishes to use light percaine in such a patient, one may soak the whole cord by placing the patient for a few minutes in each lateral position in turn. Alternatively, one may place the patient directly on his back.

Though I have used the exact Howard Jones' technique in some 400 cases with excellent results, the five to ten minutes in the prone position has always seemed to me unnecessary for the purpose of producing posterior root anæsthesia. The great value of the prone position is that one can see clearly the highest point of the spine and, therefore, the highest point to which anæsthesia will extend. The specific gravity of light percaine is much nearer to that of the cerebro-spinal fluid than is that of stovaine or heavy percaine. It follows that if after the injection of light percaine it is necessary to place a patient in the prone position

for a few minutes in order to get an effective posterior root action, the same manœuvre should be still more essential in order to obtain an anterior root effect if a heavy solution has been used. In practice, this latter is not the case. The efficiency of the anterior root effect as shown by muscular relaxation, obtained by the use of heavy solutions and without the interim use of the prone position has been appreciated for a generation. Theoretically, therefore, there should be no need for the prone position after light percaine has been given and this appears to be borne out in practice.

In a series of 50 cases to whom I gave light percaine in the lateral position and who were then turned on to their backs, only one did not have adequate sensory paralysis. In this case, the fault was almost certainly a technical one as the anterior root effect was also very imperfect.

3. " Easy control of the level of anæsthesia." This property of the Etherington-Wilson technique I must question very strongly. Apart from the evidence of Dr. Van der Post's and my own cases, I have seen very anomalous results in the hands of Etherington-Wilson technique enthusiasts such as anæsthesia well above the nipple line in a very large man, after he had been given 7 cubic centimetres percaine and had been kept erect for only 20 seconds.

4. " Quickness with which a patient can be got ready for operation." In practice, this is not a very real advantage, and no one would suggest that a minute or two is well saved if it is at the expense of the patient's margin of safety.

5. " The use of a drug which in the concentration of 1 in 1500 is decidedly less toxic than numerous of the novocaine derivatives and one, too, which does not contain alcohol." The excellence of light percaine is generally admitted, but it is hardly an argument in favour of the Etherington-Wilson technique.

Dr. Van der Post goes on to say that " it would appear that the safety factor of spinal anæsthesia is smaller than is generally believed."

I would suggest that he is confusing the safety factor of spinal anæsthesia with that of the Etherington-Wilson technique. In a personal administration of over 2,100 spinal anæsthetics, using light and heavy solutions almost equally,

but with the head in all cases placed away from the natural flow of the anæsthetic drug, I have had one death which could be fairly and directly attributed to the spinal. Of the two patients into whom I injected a light solution in the sitting position, one died and one very nearly did so. Each had previously had uneventful spinal anæsthetics, and the man who recovered was subsequently given a greater dose of the same drug though by a different method.

When I reported these cases at the Royal Society of Medicine and suggested that they provided a strong indictment of the Etherington-Wilson technique, enthusiasts for the method replied that it was unfair to blame the technique because I had not carried it out fully. Such an argument would be unanswerable had I complained of getting an insufficient height of anæsthesia. Etherington-Wilson emphasises that to make the percaine rise adequately one must take not less than 20 seconds over the injection, one must warm the ampoule to blood temperature, and one must keep the patient erect subsequently for 20 to 40 seconds according to the heights desired.[2] He states that failure to observe these details will account for inadequate or patchy anæsthesia. To play for safety and to keep the anæsthesia low, I deliberately omitted these three points. If he is right, then *a fortiori* had I followed his technique exactly, anæsthesia in these two lamentable cases would have risen still higher, if that is possible.

Dr. Van der Post suggests that the rate of diffusion of percaine in the cerebro-spinal fluid may vary with the pressure of the latter. I think there is not the least doubt that this occurs. There are also two other factors which affect diffusion and which cannot be allowed for in any particular case—the specific gravity of the cerebro-spinal fluid and the degree of obstruction present in the spinal cord. Langton Hewer[3] has said that the interior of the spinal cord probably resembles a saturated rubber sponge much more closely than the unobstructed lumen of a tube. This is possibly an overstatement, but the lumen of the spinal cord is certainly considerably obstructed and in the majority of cases the passage upwards of an injected air bubble would be very tortuous. In odd cases the passage might be perfectly free and then

the time taken for its ascent would be only a fraction of the average.

There are so many variable factors involved in the Etherington-Wilson technique of which we have little knowledge and over which we have no control that I feel the method is essentially unsound and of necessity has a narrower margin of safety than the methods it seeks to replace.

I consider that anyone using this technique is exposing his patients to an increased and unjustifiable risk.

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THE INTRODUCTION OF AVERTIN

By George Edwards, D.A.

IT is now ten years since avertin was first used in England. It also now appears that this drug, tri-bromethyl alcohol, is likely to be superseded by tri-chlor-ethyl alcohol, which is found to have greater stability in solution, to be more easily prepared and to be equally effective as a basal narcotic. These facts justify the writing of a note on the introduction of avertin into British anæsthesia.

Accounts of this new rectal anæsthetic appeared in the medical papers during 1927 and 1928, and of these the clearest was an article in *The Prescriber* of June 1927. In the early weeks of 1928 a supply of the drug in its crystalline pure form was sent by the makers, Messrs. Bayer, to the Anæsthetics Committee set up by the Medical Research Council and the Anæsthetic Section of the Royal Society of Medicine. At the same time the makers withheld the drug from the market until reports had been received and endorsed by the Committee. In London the drug was distributed to Dr. F. E. (later Sir Francis) Shipway at Guy's Hospital, to Dr. J. Blomfield at St. George's Hospital, and to Dr. F. P. de Caux. Supplies were also sent to Mr. Basil Hughes at the Municipal Hospital, Bradford, and to Mr. F. J. Morrin in Dublin. Mr. Hughes was particularly eager to try the new drug, having seen it used by Von Katzenstein in Berlin in 1927.

In the beginning the avertin crystals had first to be weighed out for each case and then dissolved in distilled water at 40°C. This was a tedious business. The patients were given at least two rectal washouts before the operation and a third afterwards. It was hardly surprising that the dispensary and nursing staffs looked upon this new method of anæsthesia somewhat critically. In the first cases, too, great efforts were made to exclude noise and distraction from the patient during induction: the wards

would be plunged into a deathly silence whenever the drug was being given.

Before long, however, we were given "avertin fluid," a 100 per cent. solution of tri-bromethyl alcohol in amylene hydrate. This made the preparation absolutely simple. We also soon learned that one enema beforehand was ample preparation and that, in emergency, even this could be omitted. It was pointed out to us by Flessa, of Munich, that the induction of narcosis was just as satisfactory, and perhaps even better, if the normal noise of the ward was allowed to go on.

The first administration of avertin in England of which we can obtain record was at St. George's Hospital, on March 20th, 1928. The patient was a healthy young woman of 27, suffering from a swelling of the submaxillary duct. She was given omnopon gr. one-third, followed 40 minutes later by avertin gr. 0.1 per kilo of bodyweight. Twenty-five minutes later still she was completely narcotised and the exploration of the floor of her mouth was carried out without difficulty and without any further anæsthetic. The patient gave one groan and a cough when the surgeon's finger went deeply into the wound. In retrospect this would not seem to have been a good case for avertinisation—particularly as it was a first case. There was no guarantee that the cough reflex would be preserved; whilst there was a certainty of intra-oral hæmorrhage. In 1938 the order of the day would be avertin induction, gas-oxygen anæsthesia, nasal intubation and pharyngeal packing. The result would be no better but, at any rate, safety would be ensured. The next case was one of epithelioma of the oesophagus for which gastrostomy was performed. Again omnopon preceded the avertin and a satisfactory anæsthesia was obtained, the patient merely moving slightly as the stomach was pulled out of the abdominal wound.

Following these two cases all types of patient were anæsthetised with avertin for all kinds of operation, and on March 16th, 1929, Drs. Blomfield and Shipway published a paper in *The Lancet* on "The Use of Avertin for Anæsthesia." This paper summarised the reports from the five anæsthetists on 198 cases. It contained accounts of two

deaths, one following Cæsarean hysterectomy under avertin in a patient suffering from advanced mitral disease and the other following removal of a massive thyroid cyst. The latter patient weighed 12 stone 11 lbs. and had a large mass filling the space between his chin and chest. He was given two-thirds of a grain of omnopon and 0.125 gr. of avertin per kilo of bodyweight. Anæsthesia was perfect but the patient died 20 hours later without recovering consciousness. The report considers that excessive dosage was the cause of death, although stating that undoubtedly the patient was a poor risk for any type of anæsthesia. This is true, but there were one or two factors in the case which we now appreciate more fully and which may have had considerable bearing on the result. First, the patient was a fat man and, therefore, could have done with less instead of more than the standard dose. Second, a large, shapeless thyroid mass was removed from a non-toxic patient and this mass probably contained all the active thyroid tissue. Thirdly, a chronically obstructed airway was freed and the respiratory centre may have been maladjusted to the carbon dioxide tension of ordinary air. The removal of the tumour was followed by an immediate improvement in respiration but this was not maintained, and had a small percentage of carbon dioxide been added to the inspired air more adequate ventilation might have been maintained during the next few hours. However, as there was no supply of the gas then available this is an idle speculation.

A fortnight prior to the publication of Drs. Shipway and Blomfield's paper in *The Lancet*, the same journal reported a paper read to the Royal Academy of Medicine in Ireland by Mr. F. J. Morrin on his experiences with avertin as a basal narcotic in throat surgery. He was particularly impressed with its value for cases in which the throat had to be treated by diathermy.

Avertin was shortly in use in hospitals all over the country and reports began to appear on all sides. Undoubtedly the one important communication was that of Dr. W. P. Kennedy, recording work which he had done at the University of Edinburgh as a Beit Research Fellow. His investigations confirmed the observation of Killian and

Uhlman in Germany that avertin narcosis could be considerably lightened and even sometimes interrupted by intravenous and intramuscular injections of coramine.

The majority of papers written on avertin have been in praise of its qualities. Most workers who have tried it have been convinced of its merits and it is now largely used in private practice and is also an established routine in some hospitals. It remains to see whether or not the chlorine compound will make the same satisfactory progress in our estimation.

THE BREAKING OF SPINAL NEEDLES

By J. N. Cave, M.B.

Anæsthetist to St. George's Hospital: Out-Patient Anæsthetist to the Victoria Hospital for Children.

THE increase in popularity of spinal anæsthesia during the past few years has led to wide discussions upon its value, the indications and contra-indications for its use and upon the constitutional effects it may have upon the patient. However, little has been said upon one other aspect of the case, namely the possibility of complications occurring during the induction of anæsthesia. On the assumption that the anæsthetist has sufficient skill and anatomical knowledge not to drive the needle through into the abdominal cavity or large vessels from behind, this resolves itself into a consideration of a not very common accident, but, nevertheless, one that is the terror of all anæsthetists—the broken spinal needle.

The first lumbar-puncture is reported as having been done by Quincke in 1889; however, it was not till some nine years later that the first accident was reported by Naumann, but even this was not due to the action of an attempt at lumbar-puncture, but was a rather peculiar accident; but it nevertheless serves to illustrate the course of action which has been followed on these occasions since they were first recorded. Naumann reports the case of a five-year-old boy who threw himself backwards on to a settee and drove a darning needle into his back. It seems to have entered at the level of the right spinous processes of the sixth and seventh dorsal vertebræ. The child developed pain in the legs and was unable to stand; his reflexes, bowels, and micturition were normal. An incision was made over the site of entry and no needle was found. Then an incision was made to the left of the spine and 3 centimetres of needle

were removed; it seemed to have been broken off by the vertebral arches. After the operation the patient was able to stand and had no further pain.

The second accident of this nature reported actually occurred during an attempt to give a spinal anæsthetic and was recorded by Torkel in 1907. In this case the patient straightened up immediately after the injection of the anæsthetic. On withdrawal the needle was found to be broken. The surgeon removed it with some difficulty before proceeding with the operation for strangulated femoral hernia. Although the needle had broken off just under the skin the difficulty arose from the fact that it had been forced deeper by the spinous process above.

The next case was reported by Landry in 1915, and occurred during the course of a diagnostic lumbar puncture. In this case the needle broke off in the canal. The patient, a man aged 38, was told of the accident. He complained of headaches which were made worse by getting up and a few days later developed pain in the lumbar region which radiated down the thighs. The fragment was localised by means of a stereoscopic X-ray and removed some 50 days after the accident, but only after an extensive search during which the spinous processes of L. 3, 4, 5 had been removed.

The failure of symptoms to occur immediately can be misleading as is shown by the case reported by Oglobina in 1928. This patient knocked his back 10 months after a spinal needle had been broken off. Up to this time there had been no symptoms. He now developed pain in the lower thoracic region and pain over D.12 was elicited by percussion. X-ray showed the needle lying on the upper part of the body of L.1 and an arthritis of D.12. At operation $3\frac{1}{2}$ centimetres of rusty needle was removed from the midline; it was 3 to 4 centimetres from the surface. After operation the patient was much better, but complained of occasional pain.

The appearance of this report by Oglobina stimulated Milko to put on record a case that occurred in 1913; the case of a boy of 16 who had a diagnostic lumbar-puncture for meningitis. The needle broke off in the canal, but he was not told of the occurrence. The meningeal symptoms

got better, but the boy had pain in the lumbar region, spreading down the left leg as far as the toes, which was of such a nature that he was unable to lie down, walk or stand, and so rapidly became very ill. The lumbar spine was fixed and there was tenderness over the spinous processes 2–5. X-ray showed the needle lying at the level of L.3, and at operation it was found that the needle was bent and had hooked itself round the nerve roots. After removal of the foreign body the patient rapidly returned to normal.

The year 1928 produced yet one more report, this time by Babcock. He removed 5 centimetres of needle from a patient quite easily about a week after the accident. The patient complained of pain for some time afterwards, but this eventually got better.

The next case is one of particular interest because the needle could not be found posteriorly and was removed by laparotomy. The patient was a woman, aged 28, who had a diagnostic lumbar-puncture for lues. The needle broke off before the spinal fluid began to flow. The only symptom recorded was a slight paræsthesia of the right leg. A lateral X-ray showed the needle lying between the bodies of L.2 and 3 with the point about 1 centimetre anterior to the vertebral column. After an abortive attempt to find the needle from behind an antero-posterior X-ray was taken and the needle was seen to lie in the mid-line between the vertebral discs, obviously penetrating the dural sac. A laparotomy was performed and the needle-point was found just to the right of the abdominal aorta and was easily removed. The patient made an uneventful recovery. This case was reported by Wieden in 1930.

In 1931 Sarnoff reported three cases in all of which the needle was removed before the onset of any symptoms, differing only in the time at which the removal took place. The first two were quite straightforward.

(*a*) The needle was removed easily immediately after the conclusion of the operation which was for a perforated gastric ulcer.

(*b*) The needle was removed easily before the operation was commenced.

(*c*) This was a much more difficult case—a boy, aged nine, was to be operated upon for acute appendicitis and peritonitis. The needle broke, but anæsthesia was obtained by injection one space higher up. A search was made for the needle, and although two spinous processes were removed it was not found. The intended operation was performed. Subsequent X-ray examination showed that there was about one inch of needle in the spinal canal. A needle was passed into the space where the needle was and a second X-ray was taken with this needle as a guide; it was found that the fragment was imbedded in the spinous process and it was removed with a groove of bone. The patient made an uneventful recovery.

In 1934 Albert Popp records the case of a woman who was going to have a hysterectomy for fibroids and who was so nervous that she suffered from considerable vomiting of a nervous origin for some days before the operation. Owing to arthritis it was impossible to introduce the needle in the lateral position, and so the patient was made to sit on the edge of the table. Entry was finally obtained below the fifth lumbar vertebra. However, the patient suddenly bent and straightened up and it was found difficult to remove the stylette. No fluid flowed. On withdrawal the needle was found to be broken. A second needle was introduced beside the first and satisfactory anæsthesia obtained. X-ray showed the broken piece of needle below L.5 and to the right of the midline. Although there were no symptoms about a month after the original operation, another spinal anæsthetic was given and the patient was placed face downward on the table which was arranged so that the convexity of the back was increased as much as possible. An incision was made over the point of the original entry and the needle easily found.

Cases Reported.

1. Dr. George Edwards, the senior anæsthetist of St. George's Hospital, has kindly given me permission to quote his notes of a case which happened at St. George's Hospital. These run as follows:

"In January 1937 I had a case to anæsthetise in hospital for an hæmorrhoid operation. The patient was 67, thin and had a chronic cough; his left leg was missing as the result of an accident some years before. I decided to use a low spinal anæsthetic. The surgeon mentioned that he had heard it said that one-legged people were apt to get pain in the stump after intrathecal injections. This, however, did not seem to be a strong enough reason to change from an otherwise perfectly suitable anæsthetic method. As a matter of fact, this absence of one leg was probably the factor which led to all the trouble. The patient was prepared in the routine way with morphia and scopolamine and for the lumbar injection I had him sitting up on the edge of the table. It was difficult to get his co-operation, for his slight deafness added to the narcotic made him almost impervious to our voices. With a lot of difficulty we got him sitting straight, the difference between his two legs tending to tip him over to the left.

"I made my puncture with a fine malleable stainless needle between L4 and 5 in the mid-line. The ligaments at first seemed very resistant, but the needle travelled satisfactorily for a further inch or so and then, after a slight increase in pressure, I felt that I was in the theca. The patient now moved and there was some difficulty in getting him back into position. On withdrawing the stilette no fluid ran. I tried to withdraw the needle slightly but found it held. Replacing the stilette was very easy except for the last half inch when a moderate amount of resistance was felt—but no more than one would expect with a slightly deflected needle. When the stilette and the needle were taken out the distal half inch of the stilette was bare.

It was obvious that the broken piece was deeply in and that attempts to locate it without skiagram would be useless; so another needle was inserted between L3 and 4 and a percaine injection given. The hæmorrhoidectomy was carried out.

Skiagrams taken on the following day showed the half inch of needle lying outside the spinal canal, pointing

forwards and slightly upwards but to the right with its point just touching the joint-space between the right superior articular facet of L5 and the inferior facet of L4.

The patient was informed of the accident and was not perturbed in any way. A week or so later the fragment was removed under local anæsthesia without incident. Furthermore, he did not complain of pain in his stump.

Inquiries showed that the needle used had been in stock for some months but had only been put into use on the previous day. It was sent with its fragment to the National Physical Laboratory and a report from that institution states that there was a definite corrosion of the metal at the site of fracture; it goes on to say "a further small round corroded area was also noticed about half an inch above the fracture. Very slight bending caused the needle to break."

In this case the probable factors which led to the fracture were (*a*) the corrosion of the needle, (*b*) the lack of alignment of the patient, (*c*) the patient moving as the needle was inserted, (*d*) the restoration of the stilette to the bent and corroded needle, and it seems probable that the actual separation of the two parts of the needle was made by the stilette being pushed through a gap in the wall of the needle itself.

2. In August 1937 I had a rather similar case. The patient was a healthy adult of 32 years of age who was to be operated upon for hæmorrhoids; as was the custom it was proposed to give the patient a low spinal anæsthetic. The patient was placed in the right lateral position and an attempt was made to perform the lumbar-puncture between L4 and 5. The inter-spinous ligament was very strong and a certain amount of pressure was needed to pierce it. As an attempt was being made to complete the puncture the patient moved. The needle was withdrawn with the idea of slightly changing the line of insertion and it was noticed that the terminal $\frac{1}{2}$ to $\frac{3}{4}$ cm. of the stilette was bare. A fresh needle was inserted in the space above and the operation was performed. The patient was informed of the accident and not only was not worried but apologised for moving.

X-ray examination the next day showed the fragment lying in the insterspinous ligament between L4 and 5 and just to the left of the midline. A few days later a search was made for it under local anæsthesia but after half an hour it was decided to leave it as it was such a small fragment and, in that position, was not likely to cause any future symptoms. If any should arise then another search could be made.

Investigation showed that the needle had been in use for some time and a physical examination of the needle revealed that it had been fairly severely twisted at one time or another.

Comments. There seem to be two main types of this kind of accident and their treatment varies slightly. In the first type the break occurs during insertion and some idea of its occurrence is got from the way the stilette jambs; in this case the stilette and needle can be left in position as a guide to broken fragment and an immediate attempt made to remove it. The second type is when the needle breaks on account of some movement of the patient after the stilette has been removed. In these cases it is usually best to X-ray the patient and attempt the removal at some later date. In these cases Sarnoff recommends passing a second needle at the site of entry of the first as an aid to localisation.

It seems that one or two routine precautions must be taken. The needles must not be left in use for very long and should be taken out of circulation if they should get severely bent at any time. The storage of the needles is very important because every care must be taken that the chances of corrosion of the metal are minimised. These are all more or less mechanical factors and should be relatively easy to control but there still remains one more factor which is not so easily controlled—the human factor as exemplified by the patient. If a spinal anæsthetic is given to a conscious patient great care must be taken that the patient realises what is about to take place and that his passive co-operation is very essential if the procedure is to be successful; it is ill-advised to give a spinal anæsthetic to a nervous patient with very little self-control unless the circumstances are such that there is no option.

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ANÆSTHETICS AT OXFORD

THE Department of Anæsthetics which, as our readers are aware, is developing under the Professor, R. R. Macintosh, whose chair was set up by Lord Nuffield, is getting into its stride. At present there are working under the Professor four assistants:

Mrs. Handfield-Jones, M.R.C.S., L.R.C.P., D.A.;
A. F. Alsop, M.A., M.R.C.S., L.R.C.P.;
F. K. Boston, M.A., M.B., B.Ch., M.R.C.S., L.R.C.P.;
Mrs. Freda B. Pratt, M.D., Ch.B.;

and the physiologist to the department, C. L. G. Pratt, M.A., M.D., M.Sc. We understand that from August 1st onwards the assistance of Dr. Erita Werler will also be available and will probably be used for following up post-operative histories and any biochemical investigations which may be found to be desirable.

THE DIPLOMA IN ANÆSTHETICS

SUBSEQUENT to the recent examination the following successful candidates have been awarded the D.A. by the Examining Board of the Royal Colleges of Physicians and Surgeons: H. N. Andrews, J. N. Cave, D. C. Clark, J. F. C. C. Cobley, J. D. Constantin, E. A. Danino, T. D. W. Fryer, J. S. Hawes, B. P. Hill, K. F. Hulbert, V. J. Keating, Barney Lawson, T. J. C. MacDonald, Marguerite R. Marshall, J. G. Mitchell, J. G. Murdoch, H. H. Pinkerton, Frances E. Radcliffe, J. A. W. Robinson, S. G. Shippard, A. M. Simson, H. L. Thornton, O. S. M. Williams, and A. H. R. Yousef.

ABSTRACTS

"The present status of intravenous administration of pentothal." R. C. ADAMS in *Canadian Med. Assoc. Journ.*, April 1938, p. 330.

A good general account of the properties, action and effects of pentothal is given. In the author's clinic pentothal was used 2,713 times in 1936. The range of operations is wide. He does not use it for children under about eight years of age, nor does he choose it for intra-abdominal work, although on exceptional occasions, of which he gives an example, it may be valuable for such operations in combination with local infiltration. Slow injection is recommended, and for long operations the needle is held in the vein, no special apparatus being recommended or found needful. In safe doses pentothal does not abolish the pharyngeal or laryngeal reflex, and is therefore not suited to operations about the throat, and equally as in safe doses it does not obliterate the abdominal and peritoneal reflexes it is not to be used as the sole anæsthetic for abdominal work. When the vein is entered 2 to 3 cubic centimetres of 5 per cent solution of pentothal are injected, the patient being instructed to count slowly. The average adult patient counts to 15. As anæsthesia deepens the breathing becomes more shallow and the state of respiration is held to be the surest guide to safety and to estimating the effect the drug is having. When respirations become noticeably deeper it is a positive sign that the anæsthesia is becoming light. From 0.5 to 1 cubic centimetre of solution injected then will bring the patient back to the previous plane of anæsthesia. The longer the duration of the anæsthesia the smaller do the subsequent doses become to produce the desired effect. No premedication is used for short operations. For larger ones the patient gets 1½ to 3 grains nembutal by the mouth about an hour before operation. Half an hour later he receives a hypodermic injection of morphia, grain 1/6, and atropine, grain 1/150. Operations lasting as long as three

and a half hours have been carried out and doses as big as 3 grains administered, though usually 1 grain (20 cubic centimetres of the 3 per cent solution) is regarded as the maximum amount needed.

"Massive collapse of the lung after cyclopropane inhalation." O. R. JONES and G. E. BURFORD, *in Journ. Amer. Med. Assoc.*, April 2, 1938, p. 1092.

The authors relate four instances of atelectasis following on anæsthesia from cyclopropane. They put forward a new explanation of this post-anæsthetic complication which has hitherto been regarded as almost always due to plugging of a bronchial tube. The authors show that collapse may occur without any such obstruction when the lung is supplied only with gases that were rapidly absorbable from the alveoli. Anæsthetic vapours, oxygen and carbon dioxide are all in this category, and if owing to the method of administration there is a complete or severe deprivation of inert gases, then collapse is likely to occur. Inert gas is normally supplied by the nitrogen of the air. It may be provided by hydrogen or by helium, and it is the last-named which the authors think should always be added to cyclopropane when this is given with high oxygen concentration, in order to avoid the risk of collapse such as occurred in the patients whose histories they record.

"Blood-pressure and respiratory volume with spinal anæsthesia." D. L. LEWIS and E. G. M. PALSER, in *Brit. Med. Journ.*, June 4, 1938, p. 1202.

The fall of blood-pressure which accompanies endothecal injection by an anæsthetic has been the source of much argument. The authors made repeated observations of the brachial, cystolic, and diastolic pressures and also of the volume of expired air, after endothecal injection of novocain. The readings obtained give information on the relation of the fall in blood-pressure to the changes in respiratory volume. The conclusions arrived at are (1) The greater part of the fall in blood-pressure is due to vasomotor paralysis. (2) A minor cause is the diminished respiratory

excursion, which is due to abdominal and intercostal paralysis rather than medullary ischæmia. (3) The Trendelenburg position has an immediate definite effect in increasing the blood-pressure, but not the respiratory volume. (4) The induction of hyperpnœa has a small delayed effect in increasing blood-pressure.

"Cyclopropane erythema."

In the same issue of the *British Medical Journal* V. J. Keating draws attention to the occurrence of erythema in two patients soon after induction of anæsthesia with cyclopropane. The rash both in appearance and behaviour was similar to that so frequently seen during ether narcosis. It is an interesting fact that every general anæsthetic, with the possible exception of nitrous oxide, appears to cause a transient rash on some occasion or another.

Divinyl ether.

There has been a considerable correspondence in the columns of the *British Medical Journal* (May 11–June 11, 1938) on the use of divinyl ether, and an admirable letter from C. F. Hadfield (June 11) gives a history of the introduction of this anæsthetic into practice as well as valuable practical hints for those unfamiliar with it. In the issue for June 11, T. A. B. Harris describes and illustrates a drip-feed which he has designed for using divinyl ether with any apparatus of the Boyle or McKesson type. Dr. Harris has used his attachment with satisfaction for a year and a half.

"Improvements in use of paraldehyde during labour." COLIN and BARTHOLOMEW, *in Amer. Journ. Obstet. and Gynaecol.*, April 1938, p. 589.

The authors have modified their technique in the use of paraldehyde in labour and report on 500 cases with the new method. The main change in this is giving the paraldehyde by the mouth instead of the rectum. Not surprisingly it is sometimes vomited. The change was made because of instances of damage to the rectum and sigmoid. It is obvious that rectal administrations on a woman at term present un-

usual inconvenience if not danger. The initial dose of paraldehyde is 24 cubic centimetres in two ounces of cold water. This is taken in sips, 15 to 20 minutes being spent over the whole dose. Three grains of nembutal were given one to two hours before. Subsequent doses of one to two drachms of paraldehyde are given if necessary. If uterine inertia is troublesome the authors employ pituitary extract which they drop on the nasal mucous membrane from a hypodermic syringe. They claim that this is a superior method to subcutaneous injection of pituitrin. The authors state that in 98 per cent of cases full amnesia is obtained by their procedure without risk to mother or child.

"Analgesia in dentistry by means of 'gas rain'." REX BINNING, in *Lancet*, May 7, 1938, p. 1050.

THE author describes a useful and simple method which can be employed to help the many patients who fear and find almost unbearable the discomfort produced by drilling teeth. It is also of service for those who are not amenable to local analgesis. The patient must be given fully to understand that he is not going to be made unconscious, merely that he will be rendered insensible to pain. There is much value in a few moments' quiet conversation beforehand in which the patient can be given confidence and instructed to breathe in through the nose and to take an extra deep breath if he feels any pain at all. Then a standard nasal inhaler is fitted to the nose allowing no leak. The expiratory valve is adjusted to a light tension and the machine is set to deliver nitrous oxide at the lowest pressure that the patient can inspire comfortably through the nose. Success in securing analgesia depends more on correct adjustment of pressure than on any other factor. The machine used, "K" model of the British Oxygen Company, delivers about 50 per cent nitrous oxide in air with the dial at the "normal" position.

Basal anaesthesia; rectal use of evipan. A. E. JONES, in *Journ. Amer. Med. Assoc.*, April 30, 1938, p. 1419.

The author differentiates three states all loosely described as basal anæsthesia: (1) Simple induction anæsthesia, with

prompt recourse to inhalation or regional methods as soon as the patient is unconscious. (2) A deeper sleep carried to the point of complete surgical anæsthesia, but constituting an auxiliary and preliminary procedure to usher in smoothly the chief anæsthetic agent under which the substantial part of the operation is to be performed. (3) A profound state of unconsciousness ushered in by suitable premedication and pushed to a plateau of tolerance which is held almost indefinitely as the backbone of the anæsthesia but is finally supplemented by brief recourse to inhalation when, in the surgeon's judgment, the limit of safety has been reached. Here the main work is done under the basal anæsthetic. It is in this sense that German authors use the term "basal anæsthesia," especially in connection with intravenous evipan anæsthesia. The writer means by basal anæsthesia "a state of unconsciousness of such degree that the patient is unaware of the events incident to the removal from bed to operating theatre and of the administration of the supplemented anæsthetic. This also implies such a degree of amnesia that there is no subsequent memory of the operation." The author describes briefly the history of evipan and the enormous number of times on which it has now been used. The 60 recorded deaths were mostly due to "errors of dosage and misunderstanding of direction." Rectal administration was tried experimentally on animals by Gwathmey who concluded that the margin of safety was very great, even when the drug was preceded by morphia. The author records the use of rectal evipan on 518 occasions, always as a basal anæsthetic before ether, ethylene, or local analgesia, and for a wide variety of surgical operations. For excretion urography in children and for removal of stitches, etc., it was used without further anæsthetic. The dosage was 0.2 c.c. of a 10 per cent. solution for every pound of body-weight, 30 c.c., for example, for a man of 150 pounds (68 kg.). That is a dose of 3 gm. evipan, a higher scale than when used intravenously.

A preliminary enema of sodium bicarbonate is given two hours before the evipan, an hour before it a hypodermic of morphia or dilaudid. The patient lies on his left side and the catheter is introduced four inches up the rectum. The

entire dose is rapidly injected and the catheter clamped. In from two to ten minutes the patient is fast asleep. In eleven instances there was confusion or delirium on waking, otherwise no unfavourable effect was noticed, and the author believes the method to be a safe and extremely convenient one.

"Morphine intravenously." C. J. BETLACH, M.D., *Proc. Staff Meet. Mayo Clinic*, November 17th, 1937.

When morphine sulphate is injected intravenously, as has been the practice in Europe since 1930, the following advantages accrue, declares C. J. Betlach, M.D., Fellow in Anæsthesia at the Mayo Foundation, Rochester: (1) Full analgesic effects are secured at once; (2) the drug is given at the very moment it is most needed; and (3) the dose may be regulated accurately.

Technique. Either ampules of a sterile solution of morphine sulphate or morphine sulphate in tablet form may be used. One-sixth or $\frac{1}{4}$ grain (10 or 15 mg.) should be dissolved in 1.5 c.c. or 2 c.c. of sterile water, respectively. This is placed in a 2 c.c. hypodermic syringe to which a 20 or 22 gauge intravenous needle has been attached. A hypodermic needle, Betlach reports, is not so satisfactory for venipuncture. Next (after venipuncture), the drug is administered very slowly, about 1/24 grain (0.25 or 0.33 c.c.) injected first, waiting for from 20 to 30 seconds before more is used. This short period, Betlach explains, will allow any idiosyncrasy to the drug, if present, to appear. After this short period, the drug is injected until the desired effect has been achieved. Only the amount necessary for analgesia should be injected.

Adjunct to regional anæsthesia. At the Mayo Clinic, intravenous injection of morphine sulphate has been used as an adjunct to regional anæsthesia, and for its analgesic effects in pre-oral endoscopy. One-sixth grain (10 mg.), given intravenously, often affords sufficient analgesia so that spinal anæsthesia lasts long enough for the surgeon to complete his work without supplementary anæsthesia. It is also "valuable" in deep cervical block, brachial plexus

block, sacral block, and other methods of regional anæsthesia. At Rochester, morphine sulphate has been administered more than 600 times for bronchoscopy and gastroscopy in the past year, with only one recording of fainting in the patient. Betlach points out that pain from trauma, visceral colics and cardiac pain can be relieved much easier by injecting morphine sulphate intravenously than subcutaneously. He also advocates its use pre-operatively in emergencies, when the regular pre-operative dose of morphine has been forgotten, or when it cannot be given soon enough.

Less vomiting. Morphine sulphate injected intravenously cannot be demonstrated in the blood 20 minutes after it first appears in it; but Betlach believes its effects last almost as long as effects of morphine sulphate used subcutaneously. Some patients report dizziness, tinnitus, tachycardia, and a feeling of warmth; but fainting with a quick recovery rarely occurs. In fact, vomiting after intravenous injection is rarer than after subcutaneous injection, because the vomiting centre is more rapidly depressed by intravenous use.

CORRESPONDENCE

To the Editor of the British Journal of Anæsthesia.

"AS OTHERS SEE US."

Sir,—The incident described by Dr. Edwards on pages 92 and 93 of the *British Journal of Anæsthesia* for April 1938 lends point to the following quotation from an article in *Anæsthésie et Analgésie*, November 1937, describing the CO_2 absorption method (p. 444).

> Enfin, lorsque l'anesthésie est obtenue et que l'anesthésiste a trouvé la bonne carburation, ce qui revient à dire qu'il est parvenu à donner la dose maximum d'oxygène compatible avec le sommeil, l'anesthésie marçhe toute seule et les anesthésistes anglais vous emmenent parfois fumer une cigarette en abondonnant leur anesthésie à son automatisme.

The writer then goes on to say that constant and unremitting attention is essential for the success of the method.

The French anæsthetist's sweeping comment on English methods is presumably based on personal observation, and it might be well for those of us who have the honour to entertain foreign visitors to consider whether the prestige of British anæsthesia is enhanced by an excess of nonchalance regarding the condition of our patients.

It is at least to be hoped that this cigarette habit is not prevalent in teaching hospitals, for whatever degree of mechanisation or automatism is adopted, the student at least should be taught to maintain constant watch on his patient.

I am, yours, etc.,

R. J. CLAUSEN.

61 Carlton Hill, N.W. 8.
June 10th, 1938.

P.S.—It is only fair to point out that the next number of *Anesthésie et Analgésie*, February 1938, contains a glowing and laudatory description of English anæsthetic methods.

It is also of interest to note that the French Society for the Study of Anæsthesia and Analgesia has maintained since January 1937 an emergency service of anæsthetists in Paris, with a central telephone number.

At the meeting of the Society on July 3rd, 1937, it was reported that not a single surgeon or specialist had up to then made use of this service! To quote from the report: "Malgré l'intérêt considérable que présente cette organisation, aucun chirurgien ou spécialist n'y a fait encore appel, tant les perfectionnements sont lents a pénétrer dans la pratique."

To the Editor of the British Journal of Anæsthesia.

THE ANÆSTHETIST'S RESPONSIBILITY.

Dear Sir,—Will you be so good as to inform me what is the legal position with regard to the responsibility of the surgeon and the anæsthetist during the performance of an operation under an anæsthetic, so far as the anæsthetic is concerned.

I am aware that in the past the surgeon was held to be responsible for everything, but am also aware that various legal rulings have relieved the surgeon from some of these responsibilities.

If there are no definite legal rulings *re* anæsthetic responsibility, what is the accepted position at the present time, and what would be likely to be the outcome of a charge of negligence in administration of anæsthetic if made against the surgeon?

Yours faithfully,

F. W. FULLERTON (M.D.).

212 New North Road,
Auckland, New Zealand.

[The position at present generally accepted, we imagine, in this country is that the surgeon is responsible for the

selection of a competent anæsthetist. After that full responsibility for everything immediately concerning the anæsthetic is the responsibility of the anæsthetist. If it came to a case at law the particular circumstances of the individual case would be investigated in each instance and might be such as to upset any general ruling.—ED.]